THE AËRONAUTICAL JOURNAL.

OCTOBER, 1904.

The Aëronautical Journal.

(PUBLISHED QUARTERLY.)

SUBSCRIPTION PER ANNUM (post free), 4s.

Communications respecting Advertisements to be addressed to the Publishers, "Aëronautical Journal,"

Messrs. KING, SELL & OLDING, Ltd.,
Lonsdale Chambers,
27, Chancery Lane, W.C.

Editorial communications should be addressed to the Editor,
53, Victoria Street,
Westminster, London, S.W.

NOTICES OF The Aëronautical Society.

At a meeting of the Council held at the Society of Arts, John Street, Adelphi, on Tuesday, July 12, 1904, the following gentlemen were elected members of the Society:—

THE RIGHT HON. LORD RAYLEIGH, D.C.L., LL.D., SC.D., F.R.S.
MR. JOHN WILLIAM DUNNE, Wiltshire Reg.
MR. NICOL FINLAYSON MACKENZIE, C.E.
MR. EUSTACE SHORT.
MR. CECIL FRANCIS WEBB.

Lord Rayleigh and Mr. William Henry Dines (late President Royal Meteorological Society) were elected members of the Council.

GENERAL MEETING.

The opening meeting of the Fortieth Session of the Aëronautical Society of Great Britain will be held at the Society of Arts, John Street, Adelphi, early in December.

The following papers will be read:—

"Kites, Kiteflying and Aëroplanes," by Mr. William Henry Dines.

"The Work of the International Aëronautical Commission," by Dr. M. H. Hergesell.

"Captive Balloon Photography," by Mr. Griffith Brewer (with lantern illustrations).

THE LIBRARY.

The first opening of the Library to members after the recess for reference and the borrowing of books will be on Monday, November 14, from two to five p.m., after which during the session it will be open on the first Monday in the month from two to five p.m., except when the first Monday falls on a bank holiday, in which case it will be open on the following Monday, or when special notice is given to members of change of date

The following books, &c., have been presented to the Library:—

By Messrs. R. Assman and A. Berson: "Ergebnisse am Arbeiten and Aëronautischen Observatorium."

By M. L. Marchis: "Legons sur La Navigation Aérienne."—Lucien Marchis.

By Mr. Eric Stuart Bruce: "Guide to St. Petersburg."

ERIC STUART BRUCE,

Honorary Secretary.

THE RETURN OF THE *DISCOVERY*.

Captain Scott has the warm congratulations of his fellow members of the Aëronautical Society of Great Britain on the safe return of the *Discovery* from the Antarctic regions.

That the expedition has been a brilliant success, not only in geographical discovery, but also in the amount of scientific observations accomplished, and the number of interesting specimens of fossils, birds, and eggs collected, is generally too well known to need much emphasis in this journal.

One achievement of Captain Scott, however, which has escaped the mention of many journals, was the transport of the captive balloon and its accessories in the *Discovery*, and its use for observation in the polar latitudes. Captain Scott was the first to take an observation balloon to polar regions, and considering every additional weight in a vessel bound for polar parts must be most carefully considered, it required no little enterprise on the part of the gifted commander to decide that the balloon and gas cylinders were worth the carrying.

The work actually done with the captive balloon has been already graphically described before the members of the Aëronautical Society of Great Britain by Lieut. H. B. Shackelton, who ascended in the balloon.

The honours bestowed on the commander of the *Discovery* are, indeed, well won. Amongst the first telegrams received was one from the King, congratulating him on the success of his expedition, and the return of the explorers. By the King's directions, a new medal is to be struck and granted to the officers and crew of the *Discovery*, in recognition of their valuable work. Commander Scott has received promotion to the rank of captain in the Royal Navy.

GENERAL MEETING.

The concluding meeting of the thirty-ninth session of the Aëronautical Society of Great Britain was held at the Society of Arts, John Street, Adelphi, on Tuesday, July 12th, 1904. The President, Major B. Baden-Powell, took the chair.

The CHAIRMAN: The Hon. Secretairy wll now read the minutes of the last meeting.

The HONORARY SECRETARY then read the minutes.

The HONORARY SECRETARY: I have been asked to announce to the members of the Aëronautical Society the dates of the two International Aëronautical Congresses to be held this autumn. First, the Congress of the International Scientific Aëronautical Commission, to be held at St. Petersburg from August 29th to September 3rd. Secondly, the International Aëronautical Congress in connection with the St. Louis exhibition. It is not necessary for me to enlarge on the advantages of the international discussion of aërial topics. I may mention that the programme of topics suggested for discussion by the respective bodies is printed in the July number of the *Aeronautical Journal*. When one views the extent of the ground to be covered in these discussions of important points at issue in the development of aëronautics, perhaps it may not be extravagant to say that any one who follows closely the questions raised in these two congresses, under the intensified light shed on them by international concourse, will learn more in a few days concerning the great problem of aërial navigation than he might discern in a lifetime of isolated study.

Then there are advantages other than the directly scientific ones which follow on the attendance at these international gatherings. One advantage, undoubtedly, is the making of lasting friendships between the thinkers of different nations, and these individual friendships may pave the way to those special friendships betwixt nations which the last century did so much to develop. It is a great satisfaction to us all that our President intends to be present at the St. Louis Congress, and I hope that several of the members of our Society will be able to take part in it, either by being present or by sending papers.

Scientific Balloon Ascents.

By Chas. Harding, F.R.Met.Soc.

I should like to preface what I am about to say with a word of explanation for the title of the present paper. The title was chosen for me by your much respected Secretary, and I merely acquiesce. The title has a charm for me, realizing how important it is for the elucidation of the many problems held in keeping for us by our atmosphere, that full advantage should be taken of balloon ascents, wherever and whenever opportunity offers, for enhancing our knowledge of the ocean of air at the base of which we live and move. Considering how popular ballooning has been now for more than a century, it is surprising how few of the ascents have been made for really scientific purposes and how few have yielded scientific results.

I have made an examination of the accounts of many balloon ascents in our own and other countries, and dealing with the ascents prior to, say, the last ten years, there seems, with very few notable exceptions, to have been little or no true gain for the advancement of science.

Ascents may be said to have been made for the last 120 years. France took the lead with the first ascent in 1783, whilst we in Englaud followed with an ascent by Vincent Lunardi in 1784, and a detailed account is published of the voyage, but from a meteorological point of view it yielded little beyond a few general temperature observations.

In 1785, Thomas Baldwin, M.A., made an ascent from Chester, and an account was published in "Airopaidia, or Aërial Recreation." Mention is made that the temperature changes were irregular, and that there was not that general decrease with increased elevation as was expected. This work contains many valuable hints and much good advice to aëronauts.

In 1804, M. Gay-Lussac made two ascents in France. Prominence was given to magnetism, but valuable observations were obtained with regard to the decrease of temperature with elevation and good hygrometic results were secured.

Between 1838 and 1850, Mr. Rush, of the Kew Observatory, under the auspices of the British Association, made 15 ascents, and attained to a considerable altitude. The chief results consist of the decrease of temperature with elevation, and specimens of air were brought down from various heights and subsequently tested. In the ascent made from Vauxhall Gardens, September 4, 1838, "a sudden and very great reduction of temperature" was experienced. In ascending with the barometer at 19 in. the air temperature was 46°, in descending with the barometer at 19 in. the air temperature was 22°, "thereby showing a reduction of temperature in the course of one hour, at the same elevation of 24°." Mr. Green who accompanied Mr. Rush on this ascent remarks: "I mention this circumstance for the purpose of directing the attention of the scientific world to those local and sudden changes of temperature which in the course of my numerous ascents I have often experienced; I have further to observe that, although the air near the earth was in a tranquil state, the current by which the balloon was chiefly operated upon (namely, S by W) must have been moving at the rate of at least 60 miles an hour."

Between 1862 and 1866, 28 ascents were made by Mr. Glaisher, under the auspices of the British Association and with the aid of advice, &c., from 14 gentlemen, all of eminent scientific standing, who formed a committee of management. The balloon chiefly used in these ascents contained about 90,000 cubic feet of gas. The principal observations were the temperature of the air and hygrometrical condition at different heights.

The observations by Mr. Glaisher have recently been before the Aëronautical Society in a paper read by Mr. Wm. Marriott, and it may seem out of place for me to further allude to them. For scientific results these ascents rank in the fore-front—there being in my opinion neither in England nor elsewhere at present anything to compare with them for scientific accuracy. They may be rivalled now by those being undertaken with such vigour and enterprise in parts of the European Continent and in America. Glaisher's observations included the determining of the actinic effects of the sun at different elevations, and an examination was made of the solar spectrum. An endeavour was made to determine the decrease of temperature with elevation in different conditions of the atmosphere, and to ascertain whether the laws of decrease generally accepted were uniform at different seasons of the year and at different times of day and night. Especial care was given to use sensitive instruments, especially thermometers,

sufficiently sensitive to give the best possible results. The observations obtained fill nearly 450 pages, and ascents were made to the height of about 7 miles. Mr. Glaisher's observations afford numerous instances of exception to the generally received decrease of temperature with increase of elevation—one instance may be given. On April 6, 1864 (ascent from Woolwich), at 10,000 feet above the earth the temperature of air was 46° at 4.40 p.m., which was practically the temperature on ground prior to ascent—and on returning to earth again. The upper current was said to be from an opposite direction to the wind at the ground, the latter being south-easterly, light; in this ascent the lowest temperature observed was 33° at 4,000 feet.

The individual ascents show very varied differences of temperature, and a few details are now given in order of month, commencing with the winter season October to March. From this it may, perhaps, be gathered whether season is a factor in the decrease of temperature with height, and the influence of the time of day may often be seen.

There were two ascents in October, both with a fairly clear sky, the first, on October 9, 1863, from 4 to 6 p.m., attained about 8,000 ft.; the decrease of temperature for the first 1,000 ft. was 4°·6, from 1,000 to 2,000 ft. the decrease was 4°·2, and the decrease afterwards became less for each 1,000 ft., declining to 1°·8 between 7,000 and 8,000ft. The other ascent was on October 2, 1865, from 6 to 8 p.m. There was a steady increase of temperature of 2°·2 in first 1,000 ft. and 1°·6 from 1,000 to 2,000 ft., the highest elevation attained. Several vertical sections of the atmosphere were made with the same result, that the lowest temperature was at the surface of the earth. The humidity was also quite abnormal from that found in ordinary ascents.

No ascents were made in November.

In December three ascents were made, and the changes of temperature were very irregular; with a clear sky the decrease of temperature ranged from 2°·3 to 4°·1 for 1,000ft., whilst with a cloudy sky the greatest decrease of temperature occurred between 2,000 and 3,000 ft., the respective values for the two ascents being 2°·5 and 3°·3, both in the afternoon; this was the highest elevation with clouds.

In January there was only one ascent between 2 and 4 p.m., the weather being cloudy or misty. The changes of temperature are very irregular, an increase being shown in the first 1,000 ft., whilst there is a decrease of 4°·7 between 1,000 and 2,000 ft., the next largest decrease was 4°·4 between 10,000 and 11,000 ft.

February has only one ascent, 2 to 4 p.m.; weather cloudy. The largest decrease of temperature was 5°·4 between 1,000 and 2,000 ft., after which the change was less for each 1,000 ft. elevation to 4,000 ft., greatest height attained; the decrease from 3,000 to 4,000 ft. was 2°·3.

In March there was one ascent, 4 to 6.30 p.m., with a clear sky. The decrease of temperature for the first 1,000 ft. was 6°·1, after which the decrease of temperature became less for each successive 1,000 ft., the change from 21,000 to 23,000 ft. being 2°, or 1° for each 1,000 ft. elevation.

Rather naturally for English ascents, the largest number were made in the summer. There were two in April, the first, 1 to 3 p.m., had a cloudy sky to 9,000 ft. The decrease of temperature for each 1,000 ft. ranged from 4°·6 between 4,000 and 5,000 ft. to 2°·4 at the highest elevation, between 8,000 and 9,000 ft. The sky was clear above 9,000 ft. The decrease of temperature for each 1,000 ft. between 9,000 and 16,000 ft. varied from 2°·0 to 2°·3, whilst above 16,000 ft. it was always less than 2°, and above 18,000 ft. less than 1° for each 1,000 ft. elevation; from 19,000 to 22,000 ft. it was 0°·5, or less for each 1,000 ft. attained. The other ascent in April, 4 to 5.30 p.m., extended to 11,000 ft.; the weather was cloudy, and the changes of temperature were decidedly irregular. A rise of 2° took place between 8,000 and 9,000 ft., and the largest decrease of temperature in a given space was, at the highest elevation 3°·4 between 10,000 and 11,000 ft.

May has only one ascent, 6 to 9.30 p.m., with a cloudy sky. The greatest height attained was 6,000 ft. The decrease of temperature for each 1,000 ft. was very regular, ranging from 3°·5 to 4°·0, notwithstanding that the ascent was made in the evening.

In June there were four ascents, the first on June 26, 1863, 1 to 2.30 p.m., with a cloudy sky throughout. The decrease of temperature for each 1,000 ft. from the ground to 3,000 ft. ranged from 5°·0 to 5°·1; it was 4°·2 from 3,000 to 4,000 ft.; 3°·7 to 3°·9 from 4,000 to 6,000 ft.; 2°·3 for each 1,000 from 6,000 to 8,000, below 2° for each 1,000 ft. from 8,000 to 19,000 ft., and

uniformly below 1° for each 1,000 above 21,000 ft. The other ascents in June did not exceed 5,000 ft., and the changes of temperature were irregular. The ascents were made between 6 and 9.30 p.m.

In July there were four ascents, the first on July 17, 1862, 10 to 12 a.m., exceeded 25,000 ft., and sky was cloudy throughout. The changes of temperature were very regular, the largest difference occurring in the first 1,000 ft., and amounting to 5°·9, each subsequent 1,000 ft. exceeded 4° to 8,000 ft., and above 10,000 ft. the decrease was less than 2° for each 1,000 ft., whilst above 22,000 ft. the decrease per 1,000 ft. was 0°·7, or less. The other July ascents were limited to 7,000 ft., and were made between 4 and 9 p.m. The decrease of temperature between the earth and 1,000 ft. was by far the largest ranging from 4°·8 to 7°·8, and there was generally a gradual decrease to highest elevation.

August has five ascents. The results obtained were very similar to those in July, and confirm the smaller difference of temperature for each 1,000 ft. at higher elevations. The highest ascent was made on August 18, 1862, 1 to 4 p.m., when the height exceeded 22,000 ft., with a clear sky throughout. The decrease of temperature for first 1,000 ft. was 7°·3, it exceeded 3° for each 1,000 to 4,000 ft., was between 2° and 2°·8 from 4,000 to 14,000 ft., less than 2° to 19,000 ft., and less than 1° for each 1,000 ft. afterwards.

In September there were four ascents, and they all confirm the results obtained in the summer months. The notable ascent of September 5, 1862, 1 to 3 p.m., gives data for a higher elevation than any other ascent. Exceptionally interesting results were secured both with sky clear and cloudy. The changes of temperature ranged from 7·2 in the first 1,000 ft., and clear sky to less than a degree for each 1,000 ft. from 27,000 to 29,000 ft. The change of temperature with cloudy sky below 7,000 ft. was appreciably less than when the sky was clear.

To summarize the temperature results obtained from the whole series of Mr. Glaisher's observations. The decrease of temperature for the first 1,000 ft. was at the rate of 1° for each 162 ft. with a clear sky and 1° for each 233 ft. with a cloudy sky. From 1,000 to 2,000 ft. the decrease of temperature was 1° for 184 ft. with a clear sky and 1° for 247 ft. with sky cloudy. Uniformly for all elevations the decrease of temperature was less with a clear sky than a cloudy sky, and the decrease of temperature in a certain elevation decreases as the height above the earth increases.

In 1869, Mr. Glaisher, by the aid of a captive balloon, made a series of observations in May to August, chiefly limited to 1,000 ft. above the earth, but in favourable conditions extending to 2,000 ft. All observations were made in the afternoon or evening. The changes of temperature were found to be larger in the afternoon than in the evening, and these observations confirm what was previously noticed, that the decrease of temperature with elevation was subject to a diurnal range.

In 1868 to 1878, Mr. Gaston Tissandier made 25 ascents in France. These give facts of some considerable interest, and the observations show that several currents of air are superimposed in the atmosphere.

In 1880 and 1881, observations were made under the auspices of the Meteorological office. The results of these ascents have not been published, but they yield interesting results confirming largely those obtained by Mr. Glaisher.

In an ascent made with Colonel Templer from Woolwich Arsenal, July 27, 1881, 2.15 to 4 p.m., I observed a rise of temperature amounting to 6°·8 in ascending from 5,420 ft. to 6,320 ft., in three minutes from 2·49 to 2·52 p.m. Thermometers were well screened from the sun's rays. In an ascent made with Mr. Walter Powell from Crystal Palace on October 28, 1881, noon to 4 p.m., several sections of the atmosphere were made. A good deal of mist or cloud was encountered and the changes of temperature were somewhat irregular, although on the whole not particularly abnormal. Weather fine, with a gentle breeze, and observations were made about every two minutes throughout the ascent.

The lamentable accident during the ascent on December 10, 1881, conducted on behalf of the Meteorological Office, in which Mr. Walter Powell, M.P., lost his life, cast a shadow on ballooning in England for nearly 20 years, and it is practically only in quite recent years that a fresh impetus is being received.

Perhaps I may be pardoned if at this point I make a personal allusion to my participation in these somewhat recent scientific ascents—ascents made with more or less thrilling experiences, but which have left me with the firm conviction that

ballooning is not fraught with real danger, provided all engaged are acting in harmony and with quiet determination.

I have had the good fortune of making ascents with Colonel Templer and with the late Mr. Walter Powell, M.P., gentlemen experienced in ballooning, and both possessing the finest qualities for securing successful ascents. One ascent was made in the ill-fated War Office balloon, "Saladin," with no less an aspiration than equalling or beating Mr. Glaisher's highest ascent, and one in Mr. Walter Powell's balloon, "The Day Star," with the intent of crossing the English Channel. This ascent was made only shortly before the loss of Mr. W. Powell, and perhaps I may be pardoned for describing the thought and care expended to avoid disaster. The balloon was built of silk and the car constructed under consultation with the Lifeboat Institution. There was a thick cork buoy encircling the car and cork seats, while the occupants (two) were each provided with lifebelts and oil skin trousers without openings at foot. There may be in this explanation a hint for similar ascents in our islands where land space is peculiarly limited.

During Mr. Glaisher's ascents from 1862 I had the good fortune to witness the ascent, at start, on more than one occasion, and I also made an ascent in the captive balloon in 1869.

In course of conversation with Mr. Powell, in mid-air, that astute observer expressed his desire to cross the Atlantic in a balloon, but suggested starting from England. I think he was soon convinced of the almost utter impossibility of reaching the United States from Europe, but our knowledge of the general wind circulation tends to favour the possibility of crossing the ocean successfully, starting from America, drifting eastwards with the general westerly winds.

There has been considerable activity with respect to balloon ascents out of England of recent years, especially in France, Germany, and the United States of America. Full justice has quite recently been done to these by Dr. Shaw in the paper which he communicated to the Society.

Our own country is not altogether idle, and Mr. Bacon's ascents promise valuable results. We are prosecuting enquiries in the upper air by means of kites, and in other countries the use of kites is seconding the method of exploration by balloons.

The perfecting of self-recording instruments renders observations now much more satisfactory, although in the case of the thermograph the screening from the sun's rays is essential.

Observations of the higher air should be invariably coupled with the synoptic weather chart covering a large area at the earth's surface—since the latter will doubtless often explain the irregular changes experienced at considerable heights. It would be of considerable scientific interest to secure as far as practicable synoptic weather charts synchronous with the several ascents made by Mr. Glaisher. I believe this to be quite possible, but it involves more time and labour than is at my disposal in connection with this paper.

May I at this point throw on the screen a few slides in illustration of the air currents a balloon is subject to, and thereby show the conditions a ballon is bound to encounter. Also a few other slides which may be of interest.

Slides were shown illustrating :—

1. The general circulation of the air in an anticyclone.
2. The general circulation of the air in a cyclonic disturbance.
3. A low barometric area wedged between two high pressure areas.
4. Movement of air in a typical cyclone.
   (*a*) At the earth's surface.
   (*b*) In the upper air.
5. A storm in Atlantic—gale force completely across the ocean.
6. Track of a storm from the Philippines, across the Pacific, the United States and Atlantic, to Western Europe.
7. Circulation of air over globe in January.
8. Circulation of air over globe in July.
9. Displacement of air on high ground.
10. A roll cumulus at sea–showing layer of air.
11. Kites and meteorograph—Blue Hill.
12. Bros. Montgolfièr.
13. Ascent from Paris, taking up sheep.
14. De Rosier at Paris—first ascent, captive.
15. M. Charles.
16. M. Charles first free ascent.
17. Lunardi, first free ascent in this country.
18. Nassau—Trio.
19. Glaisher.
20. Largest captive balloon—Paris.

The CHAIRMAN : Is there any gentleman who would like to make any remarks? I notice Dr. Robert Scott is here, and if he would make a few remarks I am sure we should be very pleased to hear him.

Dr. ROBERT SCOTT: Mr. Chairman, ladies and gentlemen,—I can only congratulate the

Society on having had such an admirably drawn up paper by Mr. Charles Harding on the subject of "Scientific Balloon Ascents." There was one set of ascents from Kew which I did not hear mentioned, made by Mr. Welsh. There were four balloon ascents which were made under the direction of a committee, and carried out by a grant from the Royal Society, but they were failures. I did not notice whether the name of Mr. Rush was given, but I thought it was exceedingly interesting to hear this summary which Mr. Charles Harding has given us.

Mr. REED: Mr. Chairman, there is one point in which I should like to differ from the author of the paper, and that is with regard to the probability of reaching America from this side. I think it is more than probable that although we have an easterly current below we may have a westerly one above. It is very often the case that we have a different direction of wind in the upper air, so I do not think the idea ought to be given up. If we relied on the lower currents alone we might be in a bad way. With regard to the rising currents of air, that is a matter of great importance to aëronauts, and it is a pity we have no data whatever on them. I do not think it would be possible to get such data from a balloon, and I think we should have to get them with kites. To the meteorologist it is interesting to have the temperature and the number of data, but to the balloonist the ascending and descending currents are of paramount importance. With regard to these observations, there are some which will be of great importance if we can get them; for instance, radio-activity at higher altitudes may teach us much, possibly it may teach us whether the store of energy we have in our earth is being dissipated into space or not. Also the composition of the air at higher levels would be of great importance to science. There is a theory that has captivated most scientific minds, that any free hydrogen is radiated into space and ultimately reaches the sun or other bodies. That is a question that may be decided by testing the air at higher altitudes. There are a great number of observations like that that might be made, and it seems a pity that although Great Britain led in the past in making these observations, yet at present we are behind Continental nations. We have not taken any official part in the series of observations with balloons, and although a great deal has been done by private enterprise, yet I really think our Government ought to wake up and take its place among the nations in making these tests.

Mr. SPENCER: Regarding the question of who was the first aëronaut to ascend in a free balloon, the first human being to whom that honour was due was Pilatre de Rosier, and not Vincent Lunardi. Pilatre de Rosier ascended in a hot-air balloon, and steered away over Paris. Then came Messieurs Robert and Charles, who ascended in a balloon inflated with hydrogen gas. Pilatre de Rosier was not only the first aëronaut to ascend in a free balloon, he was also, I regret to say, the first victim of the science, being subsequently killed in a balloon of his own construction.*

The CHAIRMAN: Perhaps Mr. Inwards will make a few remarks.

Mr. INWARDS: I am afraid, Mr. Chairman, that I have nothing to say except to repeat what other people have said, that we are all very much indebted to Mr. Harding for his very interesting paper.

The CHAIRMAN: Well I think, ladies and gentlemen, that we are all very much obliged to Mr. Harding for this paper, for he has given us a very good *résumé* of what had been done as regards meteorology from an aëronautical point of view. Of course, it is a matter of great importance for meteorologists to make observations on the air, because we are apt to take the surface of the earth and make our meteorological observations on that surface, forgetting what an enormous volume of air there is above us and what immense changes may take place in this volume of air of which we have no method of knowing what is going on. Lately, we have b een having some observations made with kites, and I am glad to hear that Mr. Dines is at the present moment occupied on one of His Majesty's ships in making meteorological observations with kites, because these kites have a special advantage even over balloons for this purpose, because you can get a very good idea of the height to which the kite flies. Now with a free balloon you practically only know the height to which it has risen by observing the barometer. You therefore cannot tell what the barometer may be doing in that particular spot, and when you talk of a balloon rising to a certain height,

* The first aëronaut to ascend in a free balloon in this country was Tytler, of Edinburgh, who ascended from that city on August 27th, 1784, in a fire balloon. Lunardi has, however, often been mentioned as the first to ascend, and he certainly made the first balloon journey of any length; ascending from London on the Artillery Grounds, Moorfields, on 15th September, 1784.—See "Aëronautica Monk Mason." Catalogue of Aëronauts, p. 247.—EDITOR.

that always is assuming that the barometer has fallen in exact accordance to what it was when it was on the ground; whereas it may have risen or fallen in that particular spot which was attained. So the kites ought to be really more valuable in that way, because you know the length of the string you have out and the angle at which it is, therefore you know the height at which it is flying. Would Mr. Harding like to make any reply?

Mr. Harding: With reference to the remarks of Dr. Scott, did he wish me to say Mr. Rush?

Dr. Scott: Mr. Welsh.

The Chairman: There are two series, I know; one by Mr. Rush and one by Mr. Welsh.

Mr. Harding: Welsh only made four ascents, that is why I have not referred to him; Rush made fifteen. With regard to the remarks about crossing to America, one gentleman remarked that it might be easterly above. I hope aëronauts will not think that a sufficiently good reason to risk their necks to try and get across. They know it is westerly below, and therefore they have no difficulty in keeping near the sea. And in such an expedition I hope they would be provided for taking to the sea. Then, Sir, with regard to the little slip which I made, I intentionally did that, and I am very glad it was noticed. I know de Rosier made the first attempt from Paris, but Lunardi made it in an ordinary balloon from this country.

The Chairman: The Honorary Secretary will now read a paper by Professor Zahm on "The Measurement of Air Velocity and Pressure." I dare say you know Professor Zahm's name as that of a gentleman who has written some very valuable works on this subject.

The Measurement of Air Velocity and Pressure.

In Aërodynamic Experiments and The Balloon Anemometer.

By PROFESSOR A. F. ZAHM.

For general service in aërodynamic experimentation two kinds of anemometer may be useful, one measuring air velocity, the other air pressure. Sometimes the velocity alone is required; again the pressure, either static or impactual, is mainly to be determined. In either case an accurate and faithful instrument is most desirable. Of course the impact can be calculated from the velocity, and *vice versâ*, providing all the data are at hand for computing the density of the moving fluid. But when many exact determinations have to be made, it is well to evade such calculations by choosing the appropriate anemometer; that is to say, a velocity instrument for measuring velocity, and a pressure instrument for measuring pressure, or impact. One thereby saves much labour, and avoids the errors that may result from misreading the elements of density.

Suppose, for example, that one is finding the head-resistance of various models in terms of the wind-speed, observing the latter by means of a velocity anemometer. The wind-speed may be kept constant throughout the research, but the resistance of any given model will vary day by day, and hour by hour. Every three degrees change of the thermometer, or eight millimetres change of the barometer, will alter the resistance one per cent. Even the variations of moisture may make a perceptible difference. So, in order to compare the resistances obtained at different periods, it is necessary to reduce them all to standard atmospheric conditions. This means a great deal of labour and many unwelcome errors. On the other hand, if a pressure anemometer is employed, all these troubles will be avoided. So long as its readings are kept constant the resistance of the models must remain constant, no matter how the atmospheric conditions may change. Thus all the observations of an extended research may be compared without reduction. In fact, for impact measurements the pressure anemometer has three distinct advantages: (1) Computations are avoided; (2) readings of temperature, pressure, and saturation are unnecessary, and errors from that source obviated; (3) the anemometer readings and model resistances may be plotted while the experiment is in progress, thus revealing observational errors which may at once be eliminated by taking new readings where required. In a similar way it may be seen that the velocity anemometer has equal advantages in many important investigations.

So the need of two kinds of anemometer is a very real one; and it would be well if they were standardized to measure accurately to, say, one per cent., throughout a wide range of velocities. At present no such instruments are offered in the market, though

possibly some of the types in use could be standardized to that degree of accuracy, particularly the screw, the cup, and the pressure-tube varieties. But thus far the calibration of such instruments has proved difficult and not completely satisfactory. Indeed it would be an advantage if the anemometers were free from empirical constants, so as not to require calibration at all.

The present paper treats of the design and use of an anemometer* whose observed indications seem to conform to those computed for it from theory. The data submitted were obtained hurriedly, as incidental to other measurements, but it is hoped they are accurate enough to establish confidence in this type of instrument; for it is easy to construct, convenient to use, adapted to a wide range of velocities, and has no empirical constant.

The anemometer is an adaptation of the Pitot tube, and consists essentially of a double pressure nozzle in connection with a delicate pressure gauge. The assembled parts are shown in fig. 1.

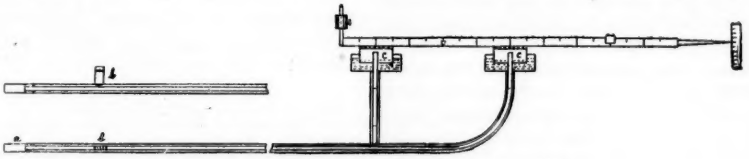

Fig. 1.—The Pressure-Tube Anemometer.

A "pressure-tube" held along stream transmits the impact pressure of the air from nozzle *a*, the static pressure from nozzle *b*, to the cups *c*, of a differential pressure-gauge. These cups, symmetrical in size and placement, are inverted over coal oil, and counterpoised from a meter-stick. A sliding weight, and a pointer, measure the difference between the static and kinetic pressures at the double nozzle; and from this can be computed the speed of the air, if its factors of density are simultaneously observed; that is, its temperature, pressure, and saturation.

We may notice some elements of construction, then the theory and test of the instrument, and finally a few of its applications.

In designing the pressure-tube it is well to select the largest pipe that can be conveniently employed, so that the air within may quickly come to rest after any displacement. The one here shown has an inch and a quarter outer pipe, a three-quarter inch inner pipe, each ten feet long and connected by three-quarter inch rubber hose to cups measuring one hundred square centimetres in cross-section. The counterpoised beam responds quickly to the varying impulses of the wind, whereas with quarter-inch hose of ten feet in length, the motion is tediously slow, owing to the great viscous resistance of the air in such narrow channels.

Almost any size and form of nozzle will convey the impact perfectly, providing it squarely face the wind. All sizes of pipe, from a lemonade straw to a milk-can show exactly the same pressure. All shapes, cylindrical, conical, hemispherical; all thicknesses of wall give identical results.

But the static nozzle must be designed with some care. It should occupy a part of the tube where the stream-lines are undisturbed, and be so shaped as not to deflect the air blowing over it when the cups are at rest. The nozzle here outlined has two longitudinal slots cut into opposite sides of the outer pipe, one foot from the end, each slot being one-eighth inch wide by three inches long. As will be shown presently, this nozzle seems to give the true static pressure of the passing wind-current.

The cups may be rigidly attached to the beam, or hung like the pans of an ordinary balance. To avoid excessive stability, owing to the buoyancy of the cup walls, it is well to make them of the thinnest metal practicable. For very delicate work it is essential that the liquid flow freely along the cup walls, so as to minimize the effect of surface tension; also the pointer excursion must be limited to a narrow range by suitable stops. If its swing is large the liquid trickling down the cup walls will materially alter the readings, unless a long time is allowed the pointer to come to rest.

The cups here shown have brass walls one hundredth of an inch thick, whose

* This instrument was exhibited before the Washington Philosophical Society, May 24, 1902.

effective buoyancy is nearly one per cent. of the pressure on the internal cup base. The coal oil flows freely along their sides, practically eliminating the objection of surface tension. The pointer swings ten millimetres to either side of the zero, the cup displacement being two millimetres. With these limited excursions the trickling of the oil is practically unobjectionable, except for the most delicate measurements. For these, of course, the pointer can be still further limited. The buoyancy, also, is unobjectionable. It is too small to impair the sensibility, and, since its effective lift per unit of surface is precisely equal to that of the air inside the cup, the total lift equals the product of the air pressure by the area of the external base of the cup.

The graduations are found to be convenient in use. The beam is a meter stick carrying a sliding weight and pointer, each of which gives the air pressure in milligrammes per square centimetre, that is to say, in millionths of an atmosphere, approximately. The scale can be made to show less than one tenth of this when the pressure is sufficiently steady, which, however, does not occur in anemometry.

So much for the details of construction and practical operation.

The hydrodynamic principle of the double pressure nozzle is contained in Bernouilli's theorem. For present purposes, however, we may suppose the density of the air constant, its flow uniform and level.

Bernouilli's theorem may then be stated as follows: at all parts of a stream-line the velocity head plus the pressure head equals a constant. Or otherwise:

$$\frac{s\,v^2}{2\,g} + p = p_0$$

in which s, v, p, are the density, velocity, and pressure of the fluid at any point of the stream-line, and p_0 is the pressure due to the total head, or pressure at a point where the velocity is zero. Hence, if the nozzle will measure the differential pressure $p_0 - p$, we can at once compute v therefrom.

To prove formula (1) let us take the general stream-line equation (Lamb's Hydrodynamics, p. 25):

$$\frac{v_1^2 - v_2^2}{2\,g} = \frac{\gamma}{\gamma - 1}\frac{p_2}{s_2}\left[1 - \left(\frac{p_1}{p_2}\right)^{\gamma}\frac{-1}{\gamma}\right]$$

v, p, s, being the velocity, pressure, density, at any one point of the stream-line; v_2, p_2, s_2, like values at any other point. Let $v_2 = o$, be the velocity at the impact mouth of the pressure tube, v_1, the velocity in the unchecked stream, and write $p_1 = p_2\,(1 - x)$. Then the general equation can be written:

$$\frac{s_1\,v_1^2}{2\,g} = \frac{\gamma}{\gamma - 1}\frac{s_1}{s_2}p_2\left[1 - (1 - x)\frac{\gamma - 1}{\gamma}\right]$$

$$= \frac{s_1}{s_2}p_2\left(x - \frac{x_2}{2\,\gamma} + \frac{1 + \gamma}{6\,\gamma^2}\,x^3 - \ldots\right)$$

Now $x = \frac{p_2 - p_1}{p_2}$, is usually very small; its largest value in this research being about 0·001; also $\frac{s_1}{s_2}$ differs from unity by less than 0·001; and $\gamma = 1{\cdot}408$. Hence the equation reduces to:

$$\frac{s_1\,v_1^2}{2\,g} \pm p\,x = p^2 - p_1$$

which is true to less than 0·1 per cent. for the range of velocities employed. Or, changing the notation, we have the formula:—

$$\frac{s v^2}{2g} + p = p_0,$$

in which p is the pressure in the unchecked stream, p_0 the pressure in the impact mouth of the tube.

To demonstrate experimentally that the pressure p_0 is equal to the total head pressure, the following plan was pursued. A tube two and one-half inches in diameter by eight inches long was inserted into the side of a tunnel in which a partial vacuum was maintained by means of an exhaust fan. A nozzle one-eighth of an inch in diameter, held in the centre of this tube, transmitted the impact of the inrushing air to one cup of the pressure gauge, the other being connected with the quiet part of the tunnel. The gauge reading was taken; then the nozzle was thrust through the inlet tube towards the outside of the tunnel, and finally clear outside. No appreciable change of pressure could be noticed, thus showing that an impact nozzle sustains a constant pressure at all points of a stream-line, and that this is equal to the total head pressure. Again, the impact pressure was read at five different fan speeds, the nozzle being held first in the quiet outer atmosphere, then at the centre of the inlet tube. The readings agreed to less than one per cent., as exhibited by the subjoined table.

TABLE I.

Comparison of Total Head with Impact Pressure at Point of Greatest Velocity.

Fan Speed.	Total Head.	Impact Pressure at Centre of Inlet.
rev. min.	mg. per sq. cent.	mg. per sq. cent.
150·	540·	538·
170·	680·	678·
200·	850·	847·5
225·	1000·	997·5
245·	1100·	1108·

If it could be shown with equal exactness that the side nozzle measures the static pressure of the fluid gliding over it, the theory of the pressure tube would be completely verified. It is easy to measure the actual pressure sustained by the side nozzle at any point of the stream, but not so easy to prove the observed pressure equal to the true static pressure at the point, except where the velocity is zero or a maximum. It is zero where the stream tube is very broad, say, in the reservoir, and here, of course, $p = p_0$; it is a maximum where the air rushes freely through the inlet tube, and here the static pressure must, according to theory, be equal to the pressure inside the tunnel. In fact, when a narrow static tube, held at the centre of a large inlet, was connected with one cup of the gauge, while the other cup was joined by hose to the quiet part of the tunnel, no material difference of pressure was observed. Hence we may conclude that the observed pressure equals the true static pressure for that particular velocity. The experiment was made at five different fan speeds with like result.

It thus appears that, for all the velocities employed, which extended from five to thirty miles an hour, the impact nozzle sustains exactly the pressure p_0, the static nozzle the pressure p. Hence when the two nozzles a, b, fig. 1, are connected simultaneously with the pressure-gauge, as is regularly done in practice, their differential pressure $p_0 - p$ must be an exact measure of the quantity $sv^2_2 g$, and therefore determines the true velocity of the air. Now, by equation (1) $sv^2 = 2g\,(p_0 - p)$; or writing $k = \sqrt{2\,g\,1_s}$, $\delta = p_0 - p$, the velocity is:

$$v = k\sqrt{\delta} \quad . \quad . \quad (2).$$

In practice the differential pressure, δ, is read directly from the instrument in milligrammes per square centimetre, and k is computed from the observed elements of density. Thus for dry air at 0° Centigrade, and a megadyne per square centimetre pressure, $s = 1{\cdot}2759$ grammes per litre, hence $k = \sqrt{2} \times 980{\cdot}95 \div 1{\cdot}2759 = 39{\cdot}21$ for the latitude of Washington; and the velocity in centimetres per second is:

$$v = 39{\cdot}21\sqrt{\delta}.$$

For air at any temperature, pressure, and humidity (Minchin's Hydromechanics), the density is:

$$s = 0{\cdot}4645\,(8p - 3f) \div 8T;$$

p being the barometric pressure in millimetres, f the aqueous vapour pressure, T the absolute temperature Centigrade. From this, k can be computed for any observed atmospheric conditions.

As a further test of the accuracy of the pressure-tube anemometer, its readings were compared with those of another instrument, designed with a special view to precision rather than convenience. Ten observations of the wind speed were taken simultaneously with both anemometers, and their average values agreed to less than one per cent. This extra instrument, which is sometimes used to standardize others, may be called the balloon anemometer. A brief description of its plan and performance may be worth a passing notice.

A toy balloon is held at the centre of the wind-tunnel, between the prongs of a fork, one prong being firmly moored to the wall by a thread, the others joined to a release string. When the string is jerked the fork spreads and the balloon floats along the wind-current without rotation. After drifting less than eight feet it has acquired the full velocity of the wind, as may be proved by projecting the balloon with like speed in still air, and observing how far it moves before coming practically to rest.

To find the velocity of the balloon two thin pencils of light are thrown squarely across its path, and the time of transit recorded by a camera designed for that purpose. The general plan of the apparatus is shown in fig. 2.

A bank of incandescent lamps, a, a, shining through $\frac{1}{8}$-inch holes in the tunnel wall, are brought to focus on a very sensitive photographic plate b. Ten feet farther along the tunnel a like thing occurs. The plates are clamped to a 10-foot board, which is drawn by hand along grooves inside the camera box, the box itself being 13 feet long. The images of the incandescent carbons trace, along each plate, fine straight lines, which are momentarily interrupted by the passing balloon.

The method is an adaptation of that used by the writer to measure the velocity of projectiles.*

To time the plate, an electric tuning-fork interrupts one of the light beams 128 times a second, making the record a dotted line. After development, the plate records are superposed, and the number of vibrations counted from centre to centre of corresponding balloon breaks. This gives the time of transit of the balloon over the 10-foot stage, from centre to centre of the light screens, and thereby determines the wind velocity. The duration of transit is revealed accurately to less than one part in five hundred. For greater exactness the circuit is broken, and the fork vibrates freely during the balloon flight.

The accompanying table gives the data for ten observations at practically the same wind velocity. Measurements were not made at other speeds for lack of time.

The results show that the average air velocities, as determined by two such widely different instruments, agree to less than

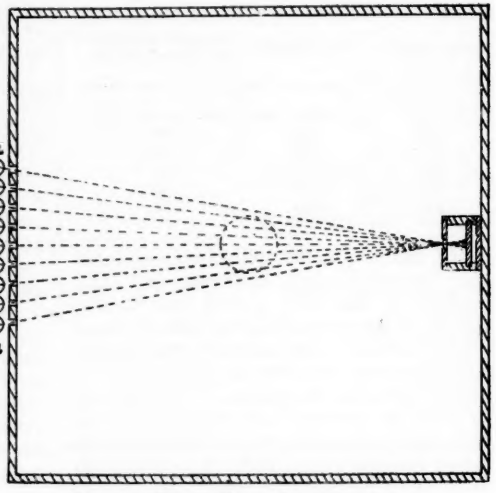

Fig. 2.—The Balloon Anemometer.

TABLE II.

Comparison of Balloon Flight and Pressure-tube Readings. Barometer, 746·51 m.m.; Temperature, 284° C.; Saturation, ·50.

Pressure-tube Readings.	Tuning-fork Vibrations.	Velocity of Wind in Tunnel.	
		By Pressure-tube.	By Balloon.
Mg. per Sq. Cm.		Ft. Sec.	Ft. Sec.
36 5	163·5	7·96	7 85
36	162·3	7·90	7·90
36	163·5	7·90	7·85
36·5	166	7·96	7·73
39·5	157·4	8 27	8·16
37·25	156·2	8·03	8·20
36	156·8	7·90	8·18
36·5	157·8	7·96	8·12
36·5	171·3	7·96	7·50
38·5	161	8·16	7·96
		Mn. 8·00	Mn. 7·95

* "Resistance of the Air at Speeds Below One Thousand Feet a Second," *Philosophical Magazine*, May, 1901.

1 per cent. The coincidence is rather too close to be representative; still, the general agreement is such as to establish confidence in this form of pressure-tube anemometer, at least for the velocity at which the test was made. To assure ourselves that the instrument is reliable at other velocities, we may recall the experimental proof that each nozzle sustains exactly the theoretical pressure at all speeds to 30 miles an hour.

Incidentally, in the course of other researches, this anemometer has been compared with several instruments of a different type; notably two cup anemometers, a large and a small one, used by the United States Weather Bureau; a screw, and a cup anemometer employed at the Smithsonian Institution; and a Dines pressure-tube anemometer.

The following page from the laboratory note-book illustrates the calibration of the first instrument mentioned. It was placed in the tunnel beside the pressure-tube in such a way that neither interfered with the other. The time of fifty revolutions of the cups was indicated by an electric bell, and the interval between two, or more, ringings of the bell was measured by means of a stop watch reading to quarter seconds. The readings of the pressure gauge were simultaneously noted.

TABLE III.

Comparison of Standard Pressure-Tube Anemometer and Four-Inch Cup Anemometer of the U.S. Weather Bureau. Barometer 30″·473 ; *thermometer* 3°·5*C.*

Date.	Fan.	Pressure-Tube.	Wind Speed.	4″Cup Anemometer.	Wind Speed.
Feb. 20, 1903.	rev. min.	mg. sq. cm.	mi. hr.	sec. in 50 rev.	mi. hr.
2.15 p.m	150	43	5·7	62·5	5·8
	200	83	7·9	44·5	7·9
	250	120	9·5	35·5	9·7
	300	180	11·7	29·0	11·7
	350	250	13·7	23·5	14·0
3.15 p.m	400	295	15·0	22·0	14·9
	450	350	16·2	19·6	16·5

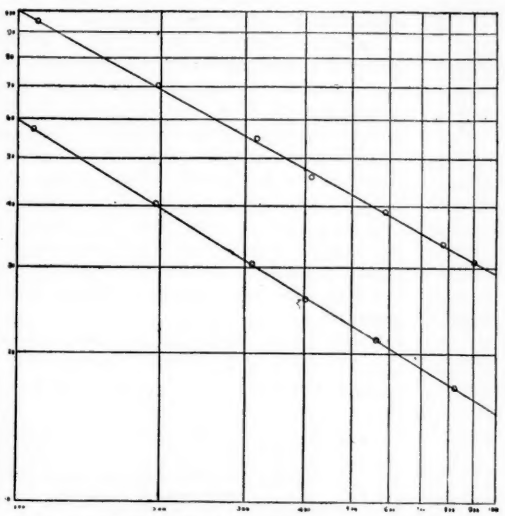

Fig. 3.—Calibration Diagram of Smithsonian Anemometers.

Abscissæ = Standard Pressure-Tube Readings in mg. sq. cm.
Ordinates = Corresponding Readings of Smithsonian Anemometers.
Cup Anemometer Diagram below; Screw-Anemometer Diagram above.

By comparing the wind velocities indicated by these two instruments it will be seen that they differ by less than 2 per cent. This is the case under favourable circumstances, when due attention is given to the working condition of the instruments ; but, if they are not very carefully used, the discrepancy may exceed 5 to 10 per cent. of the quantity measured.

In a similar way the calibration of the small Weather Bureau instrument, known as a kite anemometer, having cups one inch in diameter, and four inches from centre to centre, gave the following values :—

TABLE IV.

Comparison of Standard Pressure-Tube Anemometer and One-Inch Cup Anemometer of the U.S. Weather Bureau.

Date.	Fan.	Pressure-Tube.	Wind Speed.	Cup Anemometer.	Wind Speed.
Jan. 3, 1903.	rev. min.	mg. sq. cm.	mi. hr.	sec. in 500 rev.	mi. hr.
2.25 p.m	200	110	9.53	121·25	9 45
	300	220	13.46	83·	13.8
	400	370	17.46	65·	17·55
	500	570	21.68	51·	22.35
2.55 p.m	600	830	26.14	41·5	27·4

The results obtained in standardizing the anemometers of the Smithsonian Institution are given diagrammatically to show the concordance of the observations. They were obtained in less than an hour for each instrument, without special pains ; yet the data seem to harmonize very well, and to indicate that such instruments may give quite accurate readings when used with due care. One is a cup anemometer having cups one inch in diameter, and three and three eighths of an inch from centre to centre ; the other is a Beck screw anemometer, two and five eighths of an inch in diameter, and having flat blades. The data are plotted on logarithmetic cross - section paper for convenience, their relation being expressed by a straight line in such co-ordination.

Very interesting results were obtained from the pressure - tube anemometer invented by Mr. W. H. Dines, Fellow of the Royal Meteorological Society of England. This instrument is manufactured by Cassella, of London, and is extensively used, in one form or another, throughout Great Britain and on the Continent, notably in the meteorological service. The essential difference in principle between the Dines pressure-tube and mine consists in the form of the static nozzle, which in his instrument is a punctured dome whose axis is at right angles to the stream-lines, while in mine it is a hole, or slot, so placed as not to deflect the stream-lines.

In order to compare the two instruments, the Dines static nozzle was employed as shown in fig. 1, b^1, and readings taken at several different fan speeds. Then similar

readings were taken with the pressure tube shown in fig. 1, *b*, under the same weather conditions. The results are presented in the accompanying table, and plotted to the same scale on logarithmic cross-section paper, as shown in fig. 4.

TABLE V.

Comparison of Stream-Line Static Nozzle with Dines' Nozzle.

Fan Speed.	Pressure-Gauge Readings p^o-p		Ratio.
	With Stream-Line Nozzle.	With Dines' Nozzle.	
rev. min.	mg. sq cm.	mg. sq. cm.	
200	205	305	1·50
250	310	460	1·48
300	440	640	1·46
350	590	850	1·44
400	760	1080	1·42

The readings obtained with the Dines nozzle are approximately 1·46 times those found with the stream-line nozzle, for the velocities employed, which ranged from ten to twenty-five miles an hour. This excess is doubtless due to the suction in the rear of the punctured dome which constitutes his static nozzle. The dome, therefore, possesses the advantage of magnifying the readings of the instrument. Its hydrodynamic theory, however, is different from that of the stream-line nozzle, and its readings cannot be calculated from equation (2) of this paper.

It would be interesting to find a theoretical formula that would give, for Dines' instrument, the exact relation between the wind velocity and the differential pressure. For the range of velocities shown in Table 5, the formula $v = k^1 \sqrt{\delta}$, will apparently give the true wind speed, if 1·46 k^1 equals k of formula (2). But the comparison was too briefly made to be conclusive. Besides, the formula thus obtained is only an approximate one, and does not free the instrument from an empirical constant.

Various other forms of static nozzle have been used by different experimenters. The simplest of these is a straight, open-ended tube, standing at right angles to the current. Another form consists of two thin circular discs, placed very near together, their axis perpendicular to the current ; and in the modification used by Professor Nipher, a sheet of wire gauze fills the space between the discs, protruding slightly beyond their edges. Neither of these nozzles, however, has been compared with the one discussed in this paper.

I would refer the reader interested in the pressure-tube, as an instrument for measuring the velocity of liquids, to a very careful and valuable paper by Mr. W. M. White, read before the Louisiana Engineering Society, May 13, 1901, and published in the "Journal of Engineering Societies" the following August. His methods of experimentation were quite different from the ones here outlined, but they established conclusively the theory of the pressure nozzle for water. Indeed this is as it should be, since the equation of motion is the same in both cases. Thus the two researches corroborate one another, if the excellent work of Mr. White can be said to need any confirmation.

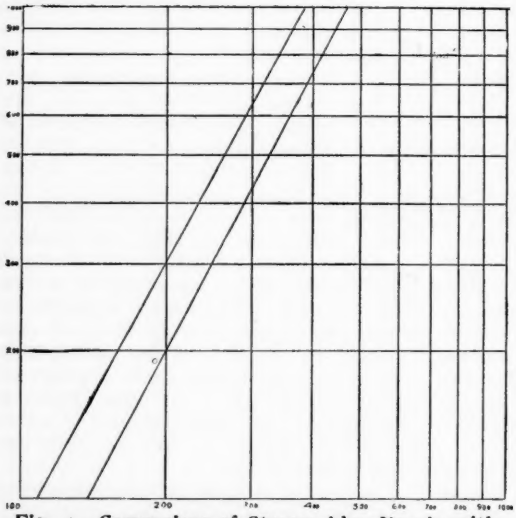

Fig. 4.—Comparison of Stream-Line Nozzle with Dines' Nozzle.

Abscissae = Revolutions of Fan per minute.
Ordinates = Readings of the Two Anemometers in mg. sq. cm.

In conclusion, it is a pleasure to acknowledge my indebtedness to Mr. D. W. Taylor, Naval Constructor U.S.N., for his kind interest in this work. Indeed it was at his suggestion that the present investigation was taken up. He was making, for the Government, a

study of the ventilation of ships, with a view to determining the efficiency of different fans, and the most favourable working conditions of the ventilating system. He relied upon the Pitot tube to determine the flow of air at various points of the ventilating pipes, and, in October, 1902, expressed the wish to have its accuracy tested in a wind current of known velocity. This led to the device which I have described as the "balloon anemometer." The apparatus was made, and the observations were taken in November, 1902.

I have also to thank Professor C. F. Marvin, of the Weather Bureau, and Mr. C. M. Manly, of the Smithsonian Institution, for many valuable suggestions, for the use of a variety of anemometers, and for the important literature and references pertaining to this work.

The CHAIRMAN: This is evidently a very important paper in its particular line. It is rather a confined subject, which we do not all thoroughly go into, and it is rather difficult to understand some of the observations when we have not the actual instruments before us. But, no doubt, those who are making a study of anemometers will be much interested in some of the details given. I may mention, incidentally, whilst we are referring to Professor Zahm, that I received the other day from him a very valuable pamphlet on skin friction, which had some very important results for those who are studying aërodynamics, in that hitherto the experiments of Langley and Maxim tended to prove that skin friction was quite negligible in air currents. But Professor Zahm —you see from the paper the method he has of going into details—has made a series of experiments which prove that the skin friction of the air on inclined surfaces is a very important consideration. That is a subject we will go into afterwards, but I thought I would just mention it, in connection with his name. As time is going on we will get on with the next paper on "Air Tight Balloons," by Messrs. Short.

An Airtight Balloon Car for High Ascents.

BY MESSRS. EUSTACE SHORT AND HORACE LEONARD SHORT.

It gives us great pleasure to have this opportunity of submitting and explaining to you a specially constructed airtight car which we have designed for the purpose of reaching high altitudes.

In the past many attempts have been made to reach great heights in open cars for the purpose of making scientific observations and experiments, and from these experiments it appears that the only difficulty which has been grappled with is that of the insufficiency of air for respiration purposes. To overcome this difficulty the general plan has been to take up in bags or other receptacles a supply of oxygen.

As no attempt, however, has been made to overcome the great inconvenience suffered by the occupants of the balloon, due to the exceedingly low pressure at these high altitudes, these many attempts, in spite of the oxygen supply taken, have been fraught with great inconvenience and danger.

The car about to be explained is designed for the express purpose of overcoming both of the aforesaid difficulties, and by carrying up a sufficient supply of air, maintained at the necessary pressure, it will enable persons to ascend without experiencing any great inconvenience, and with all their faculties clear to take observations and make experiments.

Of course there is another thing to be considered when it is desired to ascend to great heights, and that is the necessary size or capacity of the balloon. It is not necessary here to enter into this question closely, and it may be sufficient to give an approximate outline of the general features desirable for attaining this object.

Assuming the balloon is required to take the car to a height of 11 miles, where the air is approximately only one-eighth the density of that at the surface of the earth, the balloon must be able to raise itself and its complete equipment when only one-eighth part full, so as to allow for expansion without loss.

The car which forms the main subject of this paper is designed to carry three persons. Its computed weight approximates 500 lbs., and the computed weight of the envelope of the balloon, together with its complete equipment, apart from the passengers and instruments, approximate 3,300 lbs. To this must be added the weight of the three passengers, which, taken at 150 lbs. each, gives 450 lbs., say 40 lbs. for instruments, and ballast to be reserved for landing purposes, 780 lbs.

This gives a total weight of 5,070 lbs. to be raised from the ground, when the balloon is only one-eighth part inflated, and there-

fore assuming hydrogen gas with a lifting power of 65 lbs. per 1,000 cubic feet to be used, an envelope of 625,000 cubic feet capacity would be required.

The envelope, however, would be filled one-third full, so as not to be in too collapsed a form, in which it would be liable to suffer undue strain during the expansion of the gas, and therefore an extra 8,450 lbs. of ballast would be taken to regulate the ascent.

Fig. 1 shows in side elevation the proportions of the complete balloon. The envelope, which is constructed of a special fabric, is 105 feet diameter, giving a cubical capacity of 625,000 cubic

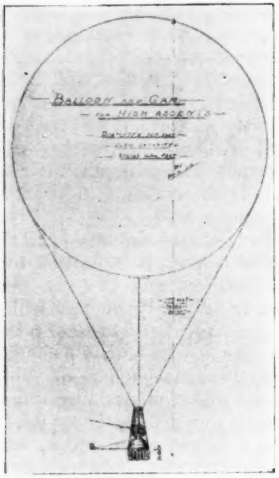

Fig. 1.

feet approximately, and approaches in form that of a sphere and fitted with exhaust above and safety valve below.

The load ring, which is of a diameter of 3 feet 6 inches and a depth of 9 inches with a thickness of $\frac{3}{8}$ inch, is constructed of ribbon steel 9 inches broad by $\frac{1}{16}$ inch thick, lapped upon itself in the form of a spiral, with 6 convolutions, the layers being firmly riveted together and lapped with canvas. This ring is 106 feet below the balloon centre, suspended from this, and at a distance of 122 feet from balloon centre is the enclosed globular car, immediately above which is a small open wicker basket; each of these are attached independently to the load ring.

Around and from the equator ring of the enclosed car is suspended by a special and independent ballast discharging mechanism the major portion of the ballast, the sole object of which is for ascentional purposes, whilst around the wicker basket is suspended the remainder of the ballast to be used solely for landing purposes.

Fig. 2 shows a sectional view of the enclosed car 8 feet in diameter, it is constructed of aluminium throughout, sp. gr. about 2·67. Means are provided for hermetically sealing the interior when the occupants are within, access and egress being through an aperture in the top, which can be firmly sealed from the inside.

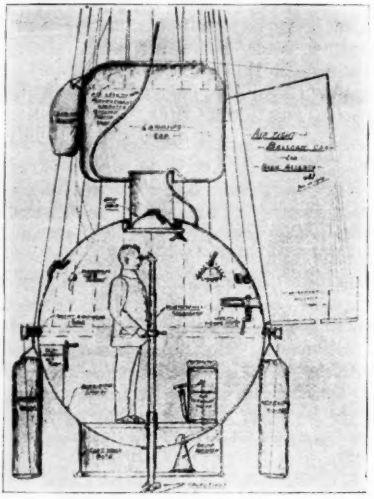

Fig. 2.

Port-holes or windows of a suitable form as shown, are provided and placed in various parts of the shell for observational purposes, each window being provided with a means of clearing away any deposit that may form on the outside of the glass and obstruct the vision; internal and external pressure gauges are also provided as well as a means of replenishing the air that it may be necessary to liberate in order to maintain a pure atmosphere within. Ballast, which is hung in bags around this car, can be discharged in predetermined quantities from the inside of the car, and scientific observations to a certain extent can be made from the interior by the means shown for this purpose. Some of the observation instru

ments being carried on the small platform projecting some distance from the shell may be viewed from the interior by the means shown in the sketch.

The wicker basket above is intended for use and observation in the lower atmospheres and communication between the two is established by a canvas tube connection.

As the supply of air to the interior of the car has to be renewed from time to time, it is quite evident that if the exterior air is to be utilised for this renovation some sort of pump will be required for pumping it in, and as the pressure of the atmosphere gradually decreases as the higher altitudes are reached, the power required to work an ordinary air pump would also have to be increased proportionately, but as we have at our command inside of the car a pressure greater than the external pressure at higher altitudes, and as this air has to be constantly renewed as aforesaid, in place of blowing it out, it can be used as an auxiliary, helping to drive a special form of air pump constructed for this purpose.

The illustration shows in a diagrammatic form a design of pump that can be utilised for this purpose, in which the air before being discharged from the inside of the car is made use of as an auxiliary power to propel the pump which supplies the inside of the car with fresh air from the surrounding media.

In the illustration, Fig. 3, A is an air cylinder fitted with two delivery valves, D V, and two suction valves, S V. The valves D V open into the inside of the car, while the two suction valves S V open to the outside of the car, to which they are connected by passages P and pipe P^1. This cylinder A is similar in all respects to an ordinary air-compressing cylinder. Opposite to this is another cylinder, which we may call the "motor" cylinder, designated by the letter M. The piston of this is connected by means of rod and a yoke piece to the piston in the air cylinder A. The valve chest of this "motor" cylinder M is open to the inside of the car. In this valve chest are two valves, and exhaust valve K, and a cut-off valve N. These valves K and N are operated by means of levers, links, &c., from the main crank-shaft Z. Valve N through the medium of a small cylinder Q, small piston X, springs S, and pipe R, which is connected to theoutside of the car, so regulates the quantity of air passing to the "motor" cylinder M as to only allow so much to escape from the car as is pumped into it by means of the cylinder A. The exhaust from this cylinder M takes place from the aperture marked E X, which is connected and delivers the exhaust from this cylinder to the outside of the car; the crank-shaft "Z" may be further operated by means of a small hand crank or foot pedal.

It will be clear that, providing the proportions between the cylinder M and the cylinder A are correctly adjusted, and also when the small governing cylinder Q is correctly adjusted and proportioned, the power required to drive the pump at any

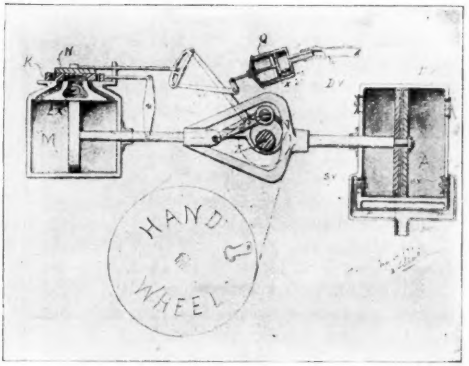

Fig. 3.

altitude will be only that required to overcome the friction of the working parts, *plus* the friction of the air itself during its entrance and exit to and from the cylinder; in fact, if a temperature can be maintained in the car of, say, 70 degrees Fahrenheit, while the temperature outside may be, say, 40 or 50 below zero, giving a total range of variation of 110, then providing that the cut-off mechanism was delicately adjusted and the frictional resistance reduced to a minimum, the pump would then be self-acting, as this range of temperature would supply the difference of the volumetric proportions to overcome the aforesaid friction. The total length over all of this pump complete as shown will not exceed 14 inches, whilst its entire weight complete would come within the limits of 12 lbs., as for

this purpose cast aluminium can be very well utilised.

It will be seen from the illustration that the constant proportion of the air exhausted and the air compressed into the car is governed by the small cylinder Q, one face of which is in connection with the exterior of the car, and the other face in connection with the interior of the car.

As the car rises, the pressure outside diminishes, whilst the pressure inside the car remains constant, consequently the internal pressure moves the piston X along the small cylinder compressing the springs at the same time, and through the lever L L it moves the fulcrum of a link, thereby so altering the travel of the valve N as to admit less air to the "motor" cylinder M, so that the volume utilized for this purpose will vary approximately equal the volume compressed.

The CHAIRMAN: Well, I think we have to thank Messrs. Short for their interesting paper. They have worked out a most ingenious scheme and seem to have gone into great detail in arranging everything that could be necessary in this case, although I do not quite know myself how the financial side of this balloon is going to be worked; because I am afraid there are always great difficulties in providing these beautiful scientific instruments, and when they are to be done on such a large scale as this I am afraid the difficulty is greater still. That is hardly a matter we need go into now, but we have to thank the authors of the paper for their kindness in submitting it to us.

Before I sit down I should like to say I received a letter from Dr. Barton a few days ago, whose sad accident you have all heard of, and I feel certain you all sympathise with him. In this letter he said he hoped he would be able to come here to-night and be able to make a few remarks about his accident, but I am sorry to hear he is not in quite a fit state to come out yet. I am sure we all hope he will soon be well and able to continue his experiments.

The HONORARY SECRETARY: I have one duty to perform, and that is on behalf of Colonel Fullerton, who was obliged to leave to catch his train. I have to propose a vote of thanks to the President for taking the chair this evening.

The vote of thanks having been carried by acclamation, the proceedings terminated.

The Development of the Aëroplane.

PAPER READ AT THE CAMBRIDGE MEETING OF THE BRITISH ASSOCIATION.

BY MAJOR B. BADEN-POWELL.

The day is undoubtedly drawing near when we shall be utilising the highway of the air for travel, and it is becoming an interesting question as to what form the motor car of the skies is to take.

A great number of abortive attempts to produce a practical apparatus have thrown considerable light on the prospects. During the last few years we have seen a great development in the construction of navigable balloons, and, in my humble opinion, these many attempts have only shown clearly what immense difficulties have to be contended with, and how little hope there is of our attaining any real success in this direction. I do not wish to imply that the navigable balloon is an utter failure, and though such a machine may prove itself most useful on certain particular occasions, and for special purposes, such as military observation, even that is a long way from being a really practical conveyance, capable of going up in all weathers, and stemming such winds as it may be likely to encounter on any average day. A balloon must, of necessity, be of huge size, and it must, therefore, offer a great resistance to rapid propulsion. Speed, however, is everything in practical aërial navigation, since the great object is to be able to progress in a given direction without interference from the wind force. If the same propelling power were applied to some smaller form of apparatus, would it be possible for the machine to maintain itself in mid-air and progress through it? Theory answers very decidedly—Yes; and even practice may point to many promising results. The aëroplane (using the word in its widest sense) may be defined as a plane, or nearly plane, surface propelled through the air in such a manner that the resulting pressure acts so as to support it against the action of gravity. The frontal resistance of such a plane, even of a very large surface, seems bound to be less than that offered by a large balloon, and should, therefore, be able to travel quicker.

Devices for attaining artificial flight may usually be classed under one of three

headings (small working models of each of which I have here):—

(*a*) Wing action, similar to a bird's flight.

(*b*) Vertically-acting screws to lift the weight upwards.

(*c*) Aëroplanes proper, or plane surfaces propelled horizontally, with a slight upward inclination.

All these three, however, are but aëroplanes according to above definition. The wing is but the aëroplane moved up and down. The vertically-acting screw is but a pair of small aëroplanes moving round a common centre.

What we have, then, to study is the action of air on inclined surfaces, and this is a subject not at all well understood.

There is another point of great importance, which has to be taken into account, and is apt to upset many theories, and that is the inertia of the air. Langley in his "Aërodynamics" clearly points this out, and shows that a horizontal plane in lateral motion falls at a slower rate than one which has no lateral motion, and that the greater the horizontal velocity the greater is the time of fall. For instance, a plane which, if dropped vertically, falls four feet in half a second will, when projected horizontally at a rate of 58 miles an hour, take two seconds to fall the four feet; that is to say, it takes four times as long in falling the same distance. This is a factor often neglected, yet it is not difficult of explanation. The plane falling vertically, having once overcome the inertia of the air, starts its downward fall carrying a current of air with it, caused both by the pressure on the particles of air underneath, and also the suction of drawing in air above. But if the plane be progressing at a great rate horizontally, it has no time, so to speak, to start any downward current of air, and is continually acting on fresh, undisturbed air.

Theory and practice are much at variance. Theorists have worked out their problems and shown what is, or should be, possible, but practice has been unable to even approximate the attainments suggested. Take the thrust of aërial screw propellers. The following are some of the records I have been able to collect:—

Nadar (in 1863), as the result of small experiments computed that a thrust of 33 lbs. should be got from one-horse power. Wenham, with a spring motor, calculated on 33½ lbs. per h. p. Dieuaide, with a small steam engine, got a pull 26·4 lbs., and Forlanini made a little engine giving exactly the same results. Vogt, a marine engineer, made some careful comparisons between aërial and sub-marine screws, finding the former produced 33 lbs. thrust per h. p. Later, with large screws rotated by manual power, he calculated that he got 55 lbs. per h. p.

Yet, when we come to trials on a large and practical scale, we find Maxim with his great steam engine of 360-horse power, and screws of 17 feet 10 inches diameter, only gets a thrust of 2,000 lbs., or at the rate of 5·7 lbs. per horse power. Santos Dumont, taking the best of several aërial engines tried, obtains a pull of 175 lbs. with 16-horse power, or 11 lbs. per horse power. Zeppelin, with two engines of like power, only gets 220 lbs. thrust, or 7 lbs. per horse power. With my own little petrol motor of 1½-horse power I have only been able hitherto to get 5 lbs. pull out of the screw. It is true that Walker in his experiments with large propellers shows from 11 up to no less than 74 lbs., but these, again, may be classed as laboratory experiments.

Commandant Renard made many trials at the French Military Aëronautical Establishment. But the results, which have not been published in full, seem to have been very variable, ranging between 48 lbs. and 17 lbs., per horse power. He remarks, significantly, that some forms of screw are so much more efficient than others and that there must be "a screw very much better than others, and its form cannot be much departed from without producing very bad aërial screws."

Then it has been usual for theorists to consider only the pressure of the air on the under surface of the plane. But recently a little more light has been thrown on the subject of what we may call the lack of pressure on the upper side, which undoubtedly has a considerable effect in "sucking" the plane upwards.

Even the pressure of the air acting on a plane surface moving in a normal direction, that is to say so that the air strikes it perpendicularly, has been variously measured. All the older text books give the formula thus:—Pressure = velocity $^2 \times$ ·005. But the more recent experiments of Langley, Renard, Dines, Stanton, and others vary from ·0027 v^2 to ·0039 v^2, showing a very marked and important difference from the old formula.

Another matter of importance on which

authorities disagree is that of skin friction. Hitherto it has been supposed that the friction of the air on surfaces moving rapidly through it was negligeable. Thus Langley in his "Aërodynamics" says, "The friction of the air is inappreciable—this fact may be stated as the result both of my own experiments and of well-known experiments of others." Maxim, making his experiments with aërial screws, tested a contrivance like a screw propeller, but with perfectly flat blades, the results of which were that he found "the skin friction between the air and the polished surface is so small that it need not be taken into consideration."

It might then have been considered that this point was settled. But no. During the last few months Mr. Zahm has read a paper before the Philosophical Society, of Washington, giving an account of a series of careful experiments he has made which seem "to prove that the frictional resistance is, at least, as great for air as for water in proportion to their densities. In other words, it amounts to a decided obstacle in high speed transportation."

It is well known that according to theory, an aëroplane should be far more efficient as a carrier of weight through the air than a lifting screw. Thus, while the lifting screw only raises the amount of its thrust, an aëroplane at a suitable inclination and practicable speed should lift about four times as much. Speaking only of my own rough experiments, I find, however, that with the same amount of propulsive power I can lift quite as much with the one as with the other. In these little models, which, however, can doubtlessly be improved upon, this fact is clearly shown.

On the other hand, it is true that Maxim with his large aëroplane, though only able to obtain a thrust of about 2,000 lbs., got a lift of some 10,000 lbs., or at the rate of 28 lbs. per horse-power. Yet this amount is what we might have expected from vertically lifting screws.

Puzzled as I have been with so much conflicting evidence and non-reliability of data, I have been at work threshing out practical results by what is known as "sheer force and stupidity." It may be said that the most successful practical flying machine is the well-known little toy butterfly, propelled with twisted india-rubber. So I started making apparatus on similar lines, getting each machine larger and heavier than the last. Several of them are here shown, the largest having a screw of 38 inches diameter, and weighing something over 3 lbs. The most efficient of them can lift a weight (for a few seconds) of 10 times the weight of india-rubber. In this way a number of facts may be collected which may prove of use in constructing still larger apparatus.

There is also another line in which I have been making trials. It seemed desirable to get some idea of how a man-carrying machine would behave while travelling in mid-air. Would, as has often been suggested, the balance be a matter of extreme delicacy and importance? What structure is necessary for the supporting aëroplanes to make them strong and rigid enough? I also wished to experiment with steering-gear methods of landing, &c. The best way of obtaining such data seemed to be to give the machine an initial impetus by running it down an incline plane and then letting it glide through the air and, to prevent serious damage to machine or man, to "land" on water. During this summer, I have had an inclined track erected beside a small lake at the Crystal Palace, and have built up a machine consisting of a canvas boat on wheels, with various forms of aëroplanes fixed above it. With this I have made a number of glides, and though the experiments are not nearly completed—for I have been delayed beyond all expectations by trivial mishaps and difficulties—a good deal has already been learnt, notwithstanding the short duration of the glides.

As regards balance. I am inclined to believe that it is not of such supreme importance. I have moved forward while in mid-air, so as to shift my weight 2 or 3 feet (which is a good deal considering the breadth of the wings was only 5 feet 6 inches), and, as far as I could tell, it did not have any excessive tilting effect. But it is especially in the constructive details that much experience has been gained. Planes stretched on two transverse bamboo poles ($1\frac{1}{2}$ inches diameter at base), about 27 feet across, proved quite strong enough to support the weight - some 270 lbs.—without any staying. But these poles bent upwards considerably, showing that they were near the limit of weakness, and would require some form of staying if rigidity were necessary. I then added an upper plane, which both took off some of the pressure from the lower planes, and also, by the structure, stiffened them.

I regret not being able to describe more

definite results, but what I hope I have shown is that more experimenting is necessary in order to obtain proper data for the construction of a flying machine, and that until we get some funds for working out these most important preliminaries, we can hardly hope to obtain really satisfactory results.

Applications for Patents.

(*Made in July, August, and September, 1904.*)

The following list of Applications for Patents connected with Aëronautics has been specially compiled for the AËRONAUTICAL JOURNAL by Messrs. BROMHEAD & Co., Patent Agents, 33, Cannon Street, London, E.C.

14897. July 2nd. JOSÉ WEISS. **Improvements relating to Flying Machines, and means for enabling the same to be tried with safety.**

15385. July 9th. EMILE MÉDÉRIC BOSSUET. **Improvements in and Relating to Dirigable Balloons, Submarine Vessels, and Wind Motors.**

15573. July 12th. DIMITRI POPESCU AND ISAK ASHIS. **Improvements in Flying Machines.**

15642. July 13th. JOSEPH WALWYN WHITE. **Improvements relating to Aerial Railways.**

15696. July 14th. LEVI SAMUEL DARLAND. **Improvements in Air-ships.**

15798. July 15th. WILLIAM PHILLIPS THOMPSON. (Communication from ALVA L. REYNOLDS and HENRY J. SEWARD, United States). **Improvements in Flying Machines.**

15933. July 18th. ANDREW BARR AND WALTER EDGAR HEARMAN. **Improved Flying Machine.**

16228. July 22nd. HENRY SHEPLEY BOOTH. **Improvements in Aerial Machines.**

16752. July 29th. COMTE VLADIMIR SKORZEWSKI. **Improvements in Balloons.**

16979. August 3rd. SAMUEL MEYER FRIEDE. **Revolving Airship Tower.**

17315. August 8th. HORATIO FREDERICK PHILLIPS. **Improvements in Flying Machines.**

19155. Sept. 5th. HENRY ALONZO BUCK. **Improvements in and Relating to Machines for Aerial Navigation.**

19313. Sept. 7th. ARTHUR EDWIN GREVILLE. **Improvements in and Relating to Aerostats or Balloons.**

19704, Sept. 13th FREDERICK HALL. **Improvement in Flying Machines.**

BY MAJOR B. BADEN-POWELL. **The Development of the Aeroplane.**